20 and 25 to 705 women under 20; 950 men between 20 and 25 to 950 women between 20 and 25; 115 men between 20 and 25 to 115 women between 25 and 30. Of the marriages of men between 25 and 30, 252 occurred where the women were under 20; 662 where the women were between 20 and 25; 284 where the women were between 25 and 30; 23 where the women were between 30 and 35. Of the marriages of men between 30 and 35, 38 occurred where the women were under 20; 112 where the females were between 20 and 25; 80 where the females were between 25 and 30; 30 where the females were between 30 and 35.

Deaths.—The abstract of the returns of deaths are given in very convenient forms, which we recommend as examples to our New York friends who may be charged with making out tabular statements of an additional year. There is first a table showing the diseases which proved fatal, the sexes and ages of the deceased, with the month when the deaths occurred, together with the places of birth. The diseases causing the deaths are arranged in alphabetical order, which is exceedingly convenient for reference. Then follow other tables, in one of which the causes of death in the several counties of the State are arranged, according to the classification recommended by the American Medical Association; whilst in another the causes of death are compared with those reported in the two previous years, 1846, 1847. The convenient form and interesting views presented in these tables reflect great credit on those concerned in making them out. The concise views presented are the results of much patient labour, as any one acquainted with the nature of statistical calculations will readily perceive. From the tables presenting the causes of death in the rural population of Massachusetts, and also that showing the influence of occupations over the chances of life, we should be glad to extract many of the interesting facts displayed, but cannot now do so, having already infringed too much upon the space allowed for a bibliograph. G. E.

ART. XXII.—*On the Cryptogamous Origin of Malarious and Epidemic Fevers.* By J. K. MITCHELL, A. M., M. D., Professor of Practical Medicine in the Jefferson Medical College of Philadelphia. Philadelphia 1849: 8vo., pp. 137. Lea & Blanchard.

THE origin of the several forms of fever that ordinarily prevail as endemics, in particular localities, or spread, occasionally, over extensive districts, as destructive epidemics, has ever been a subject of curious inquiry to the pathologist. To account for the production and spread of these diseases ingenuity has been taxed in the formation of plausible hypotheses, in evidence of the truth of each of which an array of apparently incontrovertible facts and cautious observations has been adduced by its author and advocates. Important, however, as is the subject, and industriously as it has been pursued, no one of the theories that have been heretofore advanced, can be considered as satisfactory, and we have still to confess our ignorance of the nature of the morbific cause to which fevers, whether endemic or epidemic, owe their origin. We may, it is true, be able to indicate, with some degree of certainty, a few of the leading circumstances under which one or other of the forms of fever is most liable to occur, but still the entire subject of the etiology of this class of diseases is open to investigation—and the demonstration of the cause by which they are produced, may still crown with fame the name of some industrious inquirer.

In the volume before us, Dr. Mitchell has announced a theory of the origin of malarious and epidemic fevers, which, if it be not entirely original with him, he will have the credit of having defended and illustrated by a series of facts and arguments as plausible, and apparently as conclusive, to say the least, as those upon which are based either of the theories that have preceded it. He has presented, in bold relief, the leading defects of the explanations generally received, and has attempted, with much ingenuity, to show that these defects are satisfactorily supplied by rejecting the common doctrines of the causes of malaria, and adopting the cryptogamous plants generally as the chief agent in its production.

"Of the cryptogamous plants, the *fungi*," Dr. M. remarks, "are distinguished for their *diffusion and number;* for their *poisonous properties*, and their *peculiar season of growth;* for the *minuteness of their spores*, and for their *love of darkness and tainted soils, and heavy atmospheres.*"

But while he presents the claims of the fungi as the *principal cause* of fevers, our author does "not mean to exclude the occasional agency of other cryptogamous vegetables."

Dr. M. is not so presumptuous as to suppose that he has, in the volume before us, fully demonstrated the truth of the doctrine he advocates. In undertaking the adventurous duty of developing a theoretic result, he does not expect "to do more than obtain for it, at present, a hearing and an examination; since its demonstration, if ever completed, must exact, for years, the enlightened and patient toils of many philosophers."

"Imitating," he remarks, "the natural philosophers, I have constructed a theory, not to be esteemed devoutly true, but as, in the present state of knowledge, the most perfect explanation of the known phenomena of the case; and as the least exposed to the many objections easily brought against any other hypothesis."

During a careful perusal of the several chapters in which the doctrine of the cryptogamous origin of fevers is developed by our author, we were often struck with its great plausibility, and the numerous phenomena connected with the history of epidemics, hitherto unexplained, which it readily and, apparently, correctly accounts for. That the doctrine is attended with difficulties is not, however, to be concealed. Some of the leading objections to it are noticed by Dr. Mitchell, and attempted to be removed.

One of these objections is, "that the cause assigned is not adequate to the rapid production of the effect." That it is not probable that a minute vegetable, however distributed, can contaminate the air of a large marsh or field, in the course of a few minutes or hours. To this Dr. M. replies—

"When we remember how minute a quantity of a reproductive organic virus is, in other cases, necessary to the infection of a proper subject, we might leave the argument to that defence alone; but I think there is a better one, in the wonderful growth and ready diffusion of the plants to whose nocturnal potency I am inclined to ascribe malarious fevers.

"A mushroom growth is proverbial, in every language. In a single night, under favourable circumstances, leather, or moist vegetable matter, may be completely covered with mould. Of the more minute fungi, some species pass through their whole existence in a few minutes, from the invisible spore to the perfect plant. Lind says, that the first rains in Guinea, have been known to make the leather of shoes quite *mouldy* and rotten in forty-eight hours; showing that the plants which disorganize the leather must have drawn their nutrition, even from its heart, in that time, and, by many successive generations, extended themselves over its total surface. Mr. Berkeley describes a *Polyporas squamosus* which, in three weeks, acquired a circumference of seven feet, five inches, and a weight of thirty-four pounds. The *Polyporas frondosus* described by John Bapt. Porta, sometimes transcends a weight of twelve pounds in a few days. The *Bovista giganteum*, on the authority of Carpenter, the eminent physiologist, has been known to increase in a *single night*, from a mere point, to the size of a large gourd, estimated to contain four thousand seven hundred millions of cells; a number which, when counted at the rapid rate of three hundred per minute, or five per second, would take the whole time of one person, *night and day for three hundred years.* A square mile contains upwards of 3,000,000 square yards, or 27,000,000 square feet, so that a single *Bovista giganteum* may present, at evening, an almost invisible cell, and yet place, before morning, nearly 1,800 such cells in every square foot of a square mile.

"Notwithstanding the wondrous productions of a *single* individual of *one species*, Fries, the Swedish naturalist, observed not less than *two thousand species*, within the compass of a *square furlong.* The same author tells us, that he has counted above 10,000,000 of sporules, in a single individual, of the *Reticularia maxima*, so minute as to look like smoke as they rose in the air.

"Webster, when writing of the malignant fever of 1795, informs us, that *sound potatoes from market,* perished, in his cellar, in thirty-six hours; and we know now how they perished. It was a parasitic death.

"In the *Philosophical Transactions,* Lond. (vol. iv. p. 308, Abridg.), it is stated, that a green *mould* attacked a split melon, and took three hours to sprout, and six to ripen and produce, and let fall new seeds.

"At New York, during the pestilential season of 1798, Webster says, that he saw a cotton garment covered with dark gray-coloured spots of *mildew in a single* night, and that such events were, then and there, common.

"I might multiply examples of the rapid growth and extensive diffusion of fungi, which, like the lowest class of animals, seem to have a power of development and propagation inversely as their magnitude. The more minute the plants, the more rapid their multiplication; until, as they descend to those of the smallest scale, a microscope shows them in even visible growth. Nothing astonishes one more than to see in the bottom of a watch-glass, a drop of yeast swelling up, as the *torula cerevisiæ* unfolds itself, and exhibits a forest of fungi, where, but a few minutes before, only a spore or two were visible."

Another objection to the cryptogamous origin of malarious fevers is, the difficulty of accounting for the specific identity of these in all climates, while the plants of temperate differ from those of tropical regions.

To this our author replies, that the fungi are the natural inhabitants of all the various climates of the earth. "Like man, the fungi generally live in any climate, though there are among them some that infest only the steppes of Tartary, and others that revel solely on the sands of Zahara. *This ubiquity is one of their most peculiar qualities.*"

"But why is it then," Dr. M. remarks, "if the *same* fungi create diseases in Lapland and Senegal, that there is so fatal a difference in the intensity of them at these two places? As the fungi of a poisonous character possess acrid and narcotic properties, it is scarcely necessary to consistency to presume that the same are everywhere the cause of malarious fevers. Yet, if that were an imperative supposition, it would not embarrass the question materially, because naturalists affirm, that *the poisonous cryptogami are rendered yet more poisonous by increased temperature and moisture.*"

In reference to the objection drawn from the supposed difficulty of the sporules of the fungi passing into the circulation by the lacteals, absorbents, or the pores of the venous radicles, our author states that he has found them to be at least ten times as small as the chyle globules; consequently, there can be no difficulty in their absorption, so far as their magnitude is concerned. As to the selective power of the lacteals, and other absorbents, which may be supposed to present an impediment to the absorption of the sporules of the fungi.

"We know," remarks Dr. M., "that they suffer very many and various poisons to pass into the circulation, and that, in this respect, they are much less particular than our fathers imagined. Besides this, we know that fungous growths, both in man and the lower animals, have been found in places, to which their germs could have gained access only by the circulation, or by imbibition. There is, therefore," he concludes, "no good reason for doubting that the spores of fungi find their way to the channels of the circulation, as do the cells of exanthematous diseases, and the virus of syphilis."

After noticing the fact that the cause of the uniform excess of malarious diseases, at the end of summer and autumn, has been all along an interesting subject of discussion and wonder, he proceeds to point out the error of supposing that their general prevalence then is the result of the greater amount and activity of the decomposition of dead vegetable matter going on at this season—this decomposition being in fact the most active in May and June.

"When, after the previous autumnal death, and the disintegrating effects of winter frost, or soaking rains, the warmth and moisture of spring and early summer rapidly decompose the softened textures, to feed the tender spongioles of the swelling vegetation. The great chemists, heat, light, and moisture, are then most active; and the dead relics of the former year, prepared by *time, frost,* and *rain,* are ready for the process of decomposition, as the electrical and vital agencies of the countless and thread-like *radices* open up their intended storehouses of nutrition. Although, therefore, almost every one has supposed that the autumn is the season of the greatest decomposition, that process is really conducted in the spring and early summer *with a tenfold energy*, as may be easily recognized by the extraordinary smell of the earth after a shower at this season.

"Malarious diseases, therefore, are not probably the effect of ordinary vegetable decomposition, for they occur most when that is *not at or near to its maximum.* Everywhere they abound, when the general vegetation has just passed through its great orgasm. But there is another and special vegetation, *which, whatever may be the climate, has its spring time and summer in the autumnal season* of the year. On the exhausted *debris*, and the varied *exuviæ* of plants, weeds, and grasses; from root to leaf; under ground and above ground; feed a race of vegetables which wait for *their food* to the latest period of the season of heat, and then flourish most when the more perfect forms have completed their annual task, and submit to the inroads of these Goths of phytology."

"The *insalubrity of a place has the most constant relation to the habits of the living vegetation.* Whatever may be the temperature or humidity, the most unhealthy period of the year is, in any given locality, that when the phanerogamous vegetation has completed its annual task of growth, and flowering, and fruitage, and feels the weakness of an exhausting effort; and when to triumph, as it were over a worn-out foe, the crytogamous plants plunder and destroy it."

The third lecture of Dr. M. commences with a notice of the fact that the most common malarious diseases are not producible by exposure in sickly places *during the day time;* but that, whatever may be their cause, it seems to have activity almost solely at night;—*darkness* appearing to be essential to either its existence or its power. The author then proceeds to point out the insufficiency of the received theories, in the satisfactory explanation of this curious influence of night. Rejecting the several explanations that have been advanced, he remarks:—

"But when we observe the extraordinary tendency of fungous vegetables to develop their power only at night, we detect *another analogy between malaria and the fungi.* In vain do we search in the latter part of a day for young mushrooms. The early riser finds them in their prime and abundance. A field which at evening exhibited not a single plant, is often whitened by their little umbrellas in the morning. It is well known, writes Comstock, that this tribe of plants spring up almost everywhere, especially among decaying substances, and that *thousands* may be seen in the *morning*, where none existed the evening before.

"Even the more durable kinds of fungi appear to add during the day little to their bulk, preferring to grow almost solely under the eye of night; so that these anomalous vegetables not only choose for their growth the *season of vegetable repose*, but *the hours of vegetable* SLEEP. In another respect they are beings of contrast; for, while other vegetables are adding oxygen to the air from which they have extracted its carbon, these, as if they were averse to agreeing with phenogamous plants in any respect, are eliminating carbonic acid, having extracted from the undecomposed organic matter on which they live, its more peculiar animal elements, the hydrogen and nitrogen.

"Supposing that the minutest fungi possess the general properties of the class to which they belong, we may readily perceive what prodigious influence must be exerted on them by the damp rich air of a swamp—and if they have, as Heasinger alleges, a polarizing membrane, and consequently electrical relations to the polarized vesicles of a marsh mist, that mist, imbued with moisture, enriched by the terrestrial exhalations, and screened by the shadows of night, may form the most fruitful floating soil for the invisible cells of microscopic *cryptogami:* so that from the damp earth, or the nebulous air, or both, may come out, to propagate disease, the cells of an anomalous vegetation."

To the objection that, even if the fungi do grow at night, they should, sometimes at least, taint the day air of their vicinity, from which they can scarcely be entirely eliminated by an absorbing earth or a dissipating mist: Dr M. replies, by supposing that, if they have electrical relations to the mist, or ascend only during the night, the quantity necessary to produce morbid results may not remain during the day. Other reasons for the diurnal changes of salubrity in malarious regions, he bases upon certain habitudes of the fungi.

The remainder of the lecture is devoted to an exposition of the extraordinary association, as remarked by various authors, of fungous life with the existence and propagation of great epidemics, and intense endemics.

The subject of the fourth lecture is to prove that most of the fungi are poisonous, and produce diseases resembling marsh fever.

The author commences his fifth lecture by remarking that—"A theory of malaria should not, in this enlightened age, be received, which does not, at least plausibly, account for the apparent irregularities, seeming contradictions, and anomalous inconsistencies of the subject, which now so greatly obscure all the usual modes of explanation. In this respect I hope to show the very great superiority of that which, I presume is, by this time, not unfavourably viewed by my hearers. The diffusion of the fungi; their properties as a class; their acknowledged power of producing diseases of a febrile character marked by periodicity; their nocturnal power and autumnal prevalence; their love of the damp, dark places in which febrile epidemics delight; their obvious association with many cutaneous and some mucous diseases; their production of some contagious diseases of insects; and the progress of diseases from cattle, which are sickened by eating mildewed food, to human beings, sometimes by the use of the flesh, and sometimes, as in the cases reported by Vimat, by the simple exhalation of epidemic influence; all these details, numerous, diversified, and well sustained by authorities, should, I hope, induce my auditors to advance into the subject of the present lecture with, at least, some partiality for the new doctrine."

The cause of the latency of the malarious poison, Dr. M. conceives it is not easy to comprehend, unless we suppose that the poison received into the system is organic and vital, and that the phenomena of disease depend on its modification and reaction in the body. In this way, he remarks, we can also understand how such a poison may remain dormant, like some of the animal poisons, and that its absorbed germs may be stimulated not only by time but season, following laws which we are just beginning to study. He then adduces testimony to show that the poison of the fungi may remain apparently inactive for from an hour or two to even a day and a half, and that, too, when swallowed in large quantities.

"If," he inquires, "we were now to look for any known poison as explanatory of the latency of malaria, should we not be inclined to say, that only that of the fungi exhibited in this respect a strong analogy? We *know* of no other morbific agent whose action is so uniformly and irregularly postponed."

The arrest of the progress of malaria by a low wall, a common road, or a screen of trees; the exclusive prevalence of agues on one side of a street, and the progressive inroads of malarious diseases into certain localities, Dr. M. believes, can in no manner be so satisfactorily accounted for as by supposing the morbific poison to be a fungous one, progressively marching over the soil, sustained by the rich and pregnant moisture from the marsh. We can then understand why that the wall, or the road, or the wood, should limit its progress; besides which, the spores of all fungi are more or less electrical, and are, therefore, likely to be arrested by the trees of a wood.

The fact that, in some cases, malaria is active only in the immediate neighbourhood where it originated, while in others it seems to be wafted to a great distance from its apparent source, is, according to our author, readily explained by supposing the existence of germs susceptible of reproduction and progressive growth.

"The interruption of progress by a road or wall justifies this view of the mode of conveyance, and the many facts which show the narrow limits of the poisonous activity enforce it strongly. The place, the very spot, where the disease is found, must reproduce the cause of it for itself; and if the conditions of growth are not present, then will the spot be exempt, even if very near to the most poisonous places. Thus may we, and only thus, explain the occurrence of agues, yellow fever, and cholera, on only one side of a house, or one end of a room, or one side of a street, or wall, or road. A wind may, indeed, waft the spores in small quantities to a distance, but unless there are there the conditions essential to an adequate reproduction, the spores must lie dormant and harmless. For such reproduction, the marsh mist may be one of the most important elements; but that alone will not suffice, since we know that the disease is not proportional to its frequency or intensity. Other and very local conditions seem to exercise a peculiar power. Thus, a new house is known to resist disease better than an old one, and a residence protected by an annual cultivation immediately around it, is more safe than one which is encircled by lawns in grass. During some unusually sickly years, when scarcely an inhabitant of the skirts of the city escaped marsh fever, the wind set, often for a long period, directly from the infected regions into the heart of the city. In perhaps half a minute from the time when the south-western air left the meadows and pestilential borders of the town, it had crept into every chamber of the place; yet physicians here, well know that no disease of a malarious character invaded these chambers, which were, most of them, left open during every night of the sultry autumn.

"Writers entitled to credit and authority, by position and professional character, assert, that a gauze veil, or a gauze screen in a window, adds much to the security of the wearer, or the occupant of a chamber, in even the most unsound places. We can scarcely see how any gas or vapour, simple or compound, could be arrested by such a defence; but it is easy to suppose the detention of organized and comparatively bulky bodies, electrical and glutinous or moist."

By the cryptogamous theory of fever, Dr. M. explains, with much plausibility, the greater security from disease in malarious places, derived from artificially drying the air, and the reported exemption of cooks and smiths from the sweating sickness; there being no other poison save that of the fungi, so far as we know, which is thus disarmed of its virulence by dryness and heat. By the same theory, he explains the cause of danger from sleeping in mouldy sheets, and of the sternutation caused by turning over old books and papers.

Upon no other theory can we, he believes, so satisfactorily account for the partial introduction of yellow fever, and other non-contagious malarious diseases, into places always otherwise exempted from them, by means of trunks of unwashed clothes, brought from infected regions.

"Were," he remarks, "yellow fever a contagious disease, the examples of its propagation by fomites might be easily explained; but as its non-contagiousness is clearly shown by even stronger testimony than that cited in favour of its introduction by fomites, we are left to explain the difficulty, as best we may, consistently with a belief in its importation by trunks and clothes, and a thorough conviction of its total want of contagious power. There is left but one escape, and that lies in the supposition that fungi, when lodged in the trunks, among filth and animal matter, find, in darkness and dampness, the fittest imaginable growing place. That, in scarcely any of these cases, the disease advanced beyond those who inspected or handled the clothes, is only proof of the usual difficulty of sowing successfully tropical seeds in temperate climates, and of the inaptitude of fungi to grow under any but the nicely adjusted conditions upon which many of the tribe rely."

Of a similar character, according to Dr. M., is the question of the importation of yellow fever in ships.

"A tropical fungus, carried off in dark, damp, animalized holds of ships, or in the offensive clothes of sick or dead seamen, may, he supposes, be introduced into the summer clime of unaccustomed places, and there, as it came from, may go to, the shore, and be sometimes reproductive. That the germs, when once ashore, may slowly migrate landwards, and even by chance be carried or wafted to other neighbouring spots, where they may grow, and create new *foci* of disease; that the requirements of an exotic may make such visitations rare, and such dispersions unusual, and that the equatorial plants may be nipped and even totally destroyed by an unaccustomed frost.

"Through this theory, we can easily see why the disease may be imported, why it is imported rarely, and why it makes so slow a progress from the spot to which originally brought. It will also explain its non-contagious character, and even its occasional but rare visit to a hamlet or village. It may also account for its apparently spontaneous appearance in such places as Charleston, Savannah, and New Orleans, in which the winter may not be severe enough to kill the germs, but yet may so affect them as to make their reaction difficult or partial.

"It is only thus that we can comprehend how a *perfectly healthy crew* may bring with them, in the closed hold of their ship, the germ of disease, which, after their dismissal, may pestilentially affect the 'stevedores' who discharge her, or only the labourers *who disturb her ballast*. We can thus, too, explain the *usual pause* between the first set of cases caught by visitors to, or labourers on board, the ship, and the attacks upon the inhabitants of the vicinity."—"This interval is only to be explained by the supposition that germs of some kind have gained a footing on shore, and have germinated and grown more numerous. *It is the crop in the hold which produces the first set of cases. It is the crop on the land which causes the second.*

"It is only through the action of some organic cause, that we can explain the tenacity of the attachment of yellow fever to certain ships, and these, too, among the cleanest and best aired in the British service."

By assuming for the cholera a fungous origin, Dr. M. is convinced, that all the peculiarities connected with the etiology of cholera may be readily explained. Its singular local origin; its yet more singular progress; its apparent inconsistencies; its diffusion from a tropical point over the habitable globe; its invasion, in winter, of the frozen steppes of Tartary and Russia; its solitary advance over untravelled wastes or untenanted seas, as well as the progress of the poison along the route of streams, and its preference for the damp parts of cities, are all accounted for by the assumption, that its producing cause, the *semina morbi*, are germs or spores of the cryptogami.

"The cryptogamous theory," Dr. M. observes, in his recapitulation at the close of his sixth and last lecture, "will well explain the peculiar domestication of different diseases in different regions, which have a similar climate—the plague of Egypt, the yellow fever of the Antilles, and the cholera of India. It accounts, too, for their occasional expansion into unaccustomed places, and their retreat back to their original haunts.

"Our hypothesis will also enable us to tell why malarious sickness is disproportionate to the character of the seasons; why it infests some tropical countries and spares others; why the dry Maremma abounds with fevers, while the wet shores of Brazil and Australia actually luxuriate in healthfulness. The prolonged incubative period, the frequent relapses of intermittents, and the latency of the malarious poisons for months, can only be well explained by adopting the theory of a fungous causation.

"Finally, it explains the cause of the non-recurrence of very potent maladies better than the chemical theory of Liebig, and shows why the earliest cases of an epidemic are commonly the most fatal."

Our great object, in the short analysis we have thus presented to our readers of the work of Dr. Mitchell, has been to present an outline of the doctrine of the origin of malarious fevers, advanced by the author, and a general idea of the manner in which he has explained, by the agency of poisonous fungi, and the other cryptogami, the causation of diseases of a febrile character. To appreciate fully the validity of the arguments and facts upon which the author has based his views, the whole work must be read with attention; and as it is one calculated to interest, if it does not convince the reader, there will be few, we suspect, who will not be inclined to follow the author, step by step, in his "excursions into this new field of mingled reason and fancy."

The cryptogamous theory, even though it should not be established as the true explanation of the origin of malarious and epidemic fevers, by the results of future and more extended investigation, is, at least, ingenious and plausible. Its conception, and the skill with which it is developed and sustained, are highly creditable to its author, with whom we almost feel inclined to admit, that,

"It is the most consistent with the phenomena known at present, and much better sustained by established facts than any other hypothesis yet presented to the world." That "it has, therefore, the requisites of a philosophical theory which, in other and more exact sciences, would be accepted, not to be held as absolutely true, but as, in the present state of our knowledge, the most plausible and convenient explanation of the phenomena." D. F. C.